LETTERS TO THE EDITOR.

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Observations of Mars.

IN NATURE of November 10, 1910, Mr. J. H. Worthington gives his interesting observations of the fine straight lines which he saw on Mars at Flagstaff, and expresses his belief that these "telegraph wires" are objective realities in the focal image. Although I have not seen Mr. Worthington's paper, yet I shall reply to it, basing myself on the accuracy of the abstract given of it in the Journal of the British Astronomical Association, vol. xxi., p. 130.

Now Mr. Worthington's very brief experience of the appearance of Mars during the few days spent at the Lowell Observatory is necessarily outweighed by that of an astronomer like Prof. A. E. Douglass, who spent several years in the planet's study at Flagstaff. But what was the conclusion of Prof. Douglass from his observations of the straight "canals," of which he saw more than anyone else? That they are optical "illusions" having "worked serious injury to our observations" (*Popular Science Monthly*, vol. lxx., May, 1907). It would be difficult to conceive a more decisive symptom of frailty in the "canal" question than this surrender to truth of its ablest exponent.

In discussing my work rather than the collective evidence of great telescopes (of which my results form an integral part), Mr. Worthington seems to show some misapprehension in the very object of his criticism, for my conclusions are identical with those arrived at at Lick, Yerkes, and Mount Wilson. Thus, in 1895, Prof. Barnard, summarising his evidence with the 36-inch at Mount Hamilton, said:—"No straight, hard, sharp lines were seen on the continents, such as have been shown in the average drawings of recent years" (Monthly Notices, R.A.S., vol. lvi., January, 1896, p. 166). On September 21, 1909, I state that "those geometrical spider's webs . . . do not exist" (Journal of the British Astronomical Association, vol. xx., p. 141). A fortnight later Prof. Frost telegraphs:—"Yerkes telescope too powerful for canals." Lastly, on January 3, 1910, Prof. Hale proclaims "the perfectly 'natural' appearance of the planet" in the 60-inch reflector, by far the most perfect and powerful instrument ever made, "and the total absence of straight lines" (Journal of the British Astronomical Association, vol. xx., p. 192).

It would thus appear that Mr. Worthington is perhaps attempting to revive the old controversy on the relative merits of large and small telescopes. But that question has been settled long ago, so that any attempt to renew it can no longer deserve serious consideration. The overwhelming superiority of large instruments has been often demonstrated on double stars, for the two discs seen in a great aperture will be blended, by increased diffraction, into a single mass of light with an 18-inch; and, as the smaller star is observed to revolve in perfect harmony with Newton's law, there can be no doubt whatever as to its objective existence. The same fundamental principle holds good for planetary detail. Two contiguous, irregular, bright spots on Mars in a 33-inch will appear as a single round spot in an 18-inch. Hence delicate objective markings, which are quite plain in large glasses, cannot be defined at all with inadequate instruments, and this well-known rigid demonstration establishes for ever the hopeless inferiority of small telescopes.

The advantage of great objectives I have further shown on Mars when stating (December 23, 1909) that the geometrical network vanished in perihelic opposition of the planet, while much more delicate detail was quite plain (Journal of the British Astronomical Society, vol. xx., p. 141). On September 20, 1909, under perfect seeing, I can discover no straight lines, but draw Lacus Mœris as a vast shading, and Deltoton Sinus triple (letter to Schiaparelli, dated September 21, 1909). A fortnight later the same region of Mars is photographed at Mount Wilson, and Lacus Mœris comes out likewise as a vast shading, while the triple structure of Deltoton Sinus is also confirmed. On November 3, 1909, at Flagstaff, the "lake" is missed (although covering fully one-sixth of the diameter of the planet), and Deltoton Sinus appears single, while a host of lines furrow the surface (Journal of the British Astronomical Association, vol. xx., pp. 376–7). But the fact that straight lines are drawn when more delicate detail, confirmed by photography, is missed, constitutes another proof, not only of the inadequacy of the 18-inch as compared with the 33-inch, but also of the inanity of the "telegraph wires."

Yet my position in the "canal" question should not be misunderstood. If by "canals" be meant straight lines, then I think the "canals" do not exist; if we mean irregular, more or less streaky markings, then the "canals" exist. Of course, it would be utterly illegitimate to speak of genuine canals on Mars. But in the positions of Schiaparelli's lines I often saw, with the large telescope, either (*a*) complex, irregular, knotted, or winding bands; or (*b*) jagged, isolated, dark spots; or (*c*) indented edges of differential shadings. Under good seeing, the irregularities of these objects were held steadily from five seconds to several minutes. From my experience of the "canals" since 1894, with various apertures, I am led to account for the single and double straight of lines of Schiaparelli as follows: over the objective substratum of irregular, sinuous corrugations diversifying the Martian surface, a tired eye will discover by flashes a geometrical appearance. Impressions of single lines will fleet now and then either over a narrow objective streak or over the jagged border of a half-tone, while double parallel lines will flash in the position of a broader band. But, as pointed out by Mr. Maunder, the straight lines (which, so far as my evidence goes, are usually glimpsed severally, and not collectively) are merely optical summations of groups of minute irregularities beyond the reach of the instrument used. Prof. Lowell may justly feel proud upon having succeeded where all his predecessors failed, and upon having photographed the irregular streaks of Mars by ingenious methods, devised at his observatory.

A new notion was recently introduced in science by the "born-good" and "born-bad" air of some localities; but the splendid results of Dawes, Lockyer, Burton, Green, Denning, and others in the British Isles (a country most unfavourable to telescopic work), prove that the difference between the best and worst observing stations is largely a difference of duration of good seeing. Transparency of air, which is indispensable in detecting faint stars or nebulæ, seems to be of little moment in planetary detail. When minute Martian irregularities, beyond the reach of an 18-inch at Flagstaff, are held steadily near Paris with a 33-inch; when such detail is corroborated by the unanswerable testimony of photography; and when the blue cap of Saturn is a most conspicuous feature at Meudon a whole year before the recent Solar Congress, we are bound to admit that *any* point on the earth's surface may give us short spells of perfect seeing.

E. M. ANTONIADI.

Paris, December 28, 1910.

Sir Ray Lankester's Book on the Okapi.

SIR HARRY JOHNSTON is wrong in suggesting (NATURE, December 15) that the incompleteness of my monograph of the okapi is due to the "financial control" (presumably he means the trustees of the British Museum) disliking the expense of publishing a volume of text. The full expenditure required was approved by the trustees when I was director of the museum. The absence of any further text than that which accompanies the plates and figures in the volume, as issued, is solely due to the fact that I have not provided such further text.

It would have been better to call the book "Contributions to a Knowledge of the Okapi" rather than a "monograph" of that animal, since although it is in the strict sense a monograph, it does not profess to give (as Sir Harry Johnston seems to think that word implies) a *résumé* of all that is known and has been written on

the subject. When my book was originally planned it was intended that it should be a monograph of the specimens of okapi contained in the national collection, and it thus became entered on our list as "the monograph on okapi."

More, no doubt, might be written about the specimens which I had under examination, and I should have, in some circumstances, been able to add to what the book contains; but the problems which arose in the course of my work could not, in many cases, be satisfactorily solved by the examination of the existing material.

We shall have to wait for new observations made upon fresh or living specimens for a solution of the question as to what are the characteristics of the male and female okapi respectively, what are their geographical variations, and whether there are distinct races or subspecies.

E. RAY LANKESTER.

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SIR E. RAY LANKESTER is correct in supposing that I was misled by the last paragraph of the preface to his work on the okapi into the belief that there had been or might be an additional volume of text to supplement the illustrations given in the volume under review. From private correspondence which passed between Sir E. Ray Lankester and myself about three years ago I was under the impression that the "text" alluded to was in existence, and perhaps I arrived too hastily at the conclusion that for reasons of economy it had been put aside because of the intervening publication of M. Jules Fraipont's work. The title "Monograph of the Okapi" to which Sir E. Ray Lankester refers as likely to mislead an appraiser of his work was not of my bestowal, but is the official title of this valuable and admirably produced volume. The illustrations are fully described; but I suppose what I missed, and what I hoped might still be forthcoming, were the deductions to be drawn from these illustrations as to the affinities and systematic position of Okapia: in short, a statement of Sir E. Ray Lankester's personal opinions. He is probably quite right to withhold these until something is known of the beast's musculature and intestines.

H. H. JOHNSTON.

The Dynamics of a Golf Ball.

WITH a view to reproduction in the forthcoming Life of the late Prof. Tait, I have just been editing his popular article on long driving, which appeared in the *Badminton Magazine* of March, 1896. On reading Sir J. J. Thomson's lecture, as published in NATURE of December 22, 1910, I was greatly struck with the strong resemblance between golf-ball paths worked out mathematically by Tait and the stream lines of the electrified particles in the ingenious experiment devised by Sir J. J. Thomson. A few of Tait's calculated curves were given in NATURE, vol. xlviii. (June 29, 1893); but better examples will be found in the second paper on the path of a rotating spherical projectile (Trans. R.S.E., vol. xxxix., or Scientific Papers, vol. ii., p. 386) and in the article on long driving already mentioned.

By laborious arithmetical calculations, Tait and his assistant computer worked out a series of possible trajectories with various values for the transverse force due to the underspin, obtaining, among others, the kinked path which Tait had already demonstrated by undercutting a light rubber balloon. It is extremely interesting to see how the several types of curve figured by Tait for the same initial speed of projection, but varying degrees of underspin, are almost accurately reproduced by Sir J. J. Thomson's beautiful method of subjecting a stream of negatively charged particles to a suitable combination of electric and magnetic forces.

C. G. KNOTT.

Edinburgh University, January 2.

On the Simultaneity of Abruptly-beginning Magnetic Storms.

I WAS naturally much interested in Dr. Krogness's communication to NATURE of December 8, 1910 (p. 170), and wish to take this occasion to express my gratefulness to him for making known his criticisms on some of the results of my investigations on magnetic storms, as well as on those of Mr. Faris, where there is opportunity for reply. I am also glad that he has made his statements sufficiently direct, so as to admit of an equally direct answer.

Dr. Krogness first wishes to show that my conclusion, that even the sudden magnetic disturbances do not begin strictly at the same instant, but at measurably different times at various points on the earth, rests on insecure foundation; he would make it appear that it was based on but two cases, viz. the disturbance of May 8, 1902, and that of January 26, 1903. He will find a table (No. VIII.) in No. 2 of my researches (December, 1910, issue of *Terrestrial Magnetism and Atmospheric Electricity*) which summarises the data from thirty-eight abruptly-beginning disturbances between the years 1882 and 1909, thirty-four of which were available to me when the article was prepared which Dr. Krogness reviews (*loc. cit.*, pp. 19–20).

The table gives the date and approximate Greenwich mean civil time for each of these thirty-eight disturbances, next the number of observatories for which time data were available and the approximate portion of a complete circuit of the earth embraced by the contributing observatories. Then the value of x, or the time in minutes required by a disturbance to pass over one-fourth of a great circle, and in the following columns is given the approximate weight to be attached to any particular value of x, as determined from all circumstances involved, and the source from which the data have been obtained. A plus sign attached to x means that the disturbance progressed apparently in an eastwardly direction, as indicated by an increase in the Greenwich mean time of beginning at easterly stations over that at westerly ones. A minus value of x means, of course, the reverse. Nos. 35–38 were since added on the basis of data communicated by Mr. Faris (*loc. cit.*, pp. 213, 214).

Out of thirty-eight values of x, only ten, or about one-fourth, have the negative sign, so that three-fourths of the disturbances of the type here considered show an eastward progression at the times of beginning. In view of the greatly varying circumstances on which the figures are based—different observatories, different instruments, times scaled by different persons, different years, covering a period of two and a half times that of a sun-spot cycle—it is going to be difficult to explain the persistency of the plus sign by any such possible errors as Dr. Krogness points out, which, as a matter of fact, even he will hardly contend would be always in the same direction for *every* observatory, nor even necessarily always the same at the same station.

From this table the following results are derived:—

Weighted mean value of 28 plus values of x	$= +1{\cdot}65$ minutes
,, ,, 10 negative ,,	$= -1{\cdot}80$,,
Weighted mean without regard to sign ...	$= \pm 1{\cdot}69$,,
(Hence velocity of progression for average sudden disturbance, whether to the east or to the west, is	99 km./sec.)
Weighted mean with regard to sign	$= +0{\cdot}74$ minute
(Hence average *algebraic* velocity of eastwardly progression is	225 km./sec.)

We thus get a velocity for the progression of a sudden disturbance on the order of 100 to 200 kilometres per second; hence, if a sudden disturbance passed around the earth completely it would take approximately between seven and three minutes. We are here, then, dealing apparently with a velocity of a greatly subordinate order (1/3000 to 1/1500) to that of electromagnetic waves, which would require but a tenth of a second to pass round the earth, and of kathode rays which would take on the order of a half-second.

Another line of argument set forth in my papers is based on the harmonic analysis of the typical disturbance here under consideration, for which the effect, in general, is an increase in H (horizontal intensity) over the whole earth and a decrease in Z (vertical intensity) in the northern magnetic hemisphere and an increase in Z in the southern. It was found, for example, that the disturbance system of